

Karen Shashok

Translator and Editorial Consultant,
Granada, Spain

Correspondence to:
Compositor Ruiz Aznar, 12, 2-A.
18008 Granada, Spain.
Fax: +34-958132354.
E-mail: kshashok@ctv.es

The Baltimore affair: a different view

You've heard it all before, perhaps: the epic story of an uncooperative lab director with communication problems; a disenfranchised and disillusioned whistleblower with a mission, a world-renowned but obstinate scientist; a powerful congressman with a mandate to ensure that the taxpayer's money is used responsibly; a duo of unofficial but intellectually well-armed and ethically uncompromising investigators; confused university officials torn between concerns about fairness on one hand and possible damage to their institution's reputation on the other; government officials more preoccupied with in-fighting and advancing their own careers than with their investigative duties; aggressive and unscrupulous Washington lawyers; friends, enemies, colleagues, emotions, loyalties, moral obligations and self-interest. In short, this was a very human story about people who were expected to behave in a superhuman way under exceptional circumstances. All were convinced that their motives were honorable; all were under pressure to protect their own reputations in the eyes of different peer communities and constituencies; and all made serious mistakes.

Of the thousands of lines that have been written about the Baltimore affair (also known as the Imanishi-Kari affair), two books that attempt to trace the full story and place the events into perspective are worth reading for those who require a more cool-headed view than what was provided from contemporaneous journal, newspaper and television coverage of the controversy. One book has been reviewed in prominent journals [7, 10, 11]; the other has received much less attention.

The origin, development and outcome of the Baltimore case are documented in detail in both books, which are written for the educated general public. Daniel J. Kevles, a historian of science at the California Institute of Technology (Caltech), has produced a book (Fig. 1) that has been cited as the definitive study, and as conclusive evidence of the dangers of government interference in the ethical oversight of the research process [3]. His seventeen-chapter study is accompanied by a glossary of technical terms and of source abbreviations, copious endnotes, and an essay on sources. He goes into great detail on the errors made by the Office of Science Integrity (OSI) (now the Office

of Research Integrity, ORI) and the Secret Service in their analyses of the evidence against Imanishi-Kari, and attacks John Dingell and his staff for being overly concerned—for their own political motives—with teaching arrogant scientists that they are obliged to use public money with utmost responsibility.

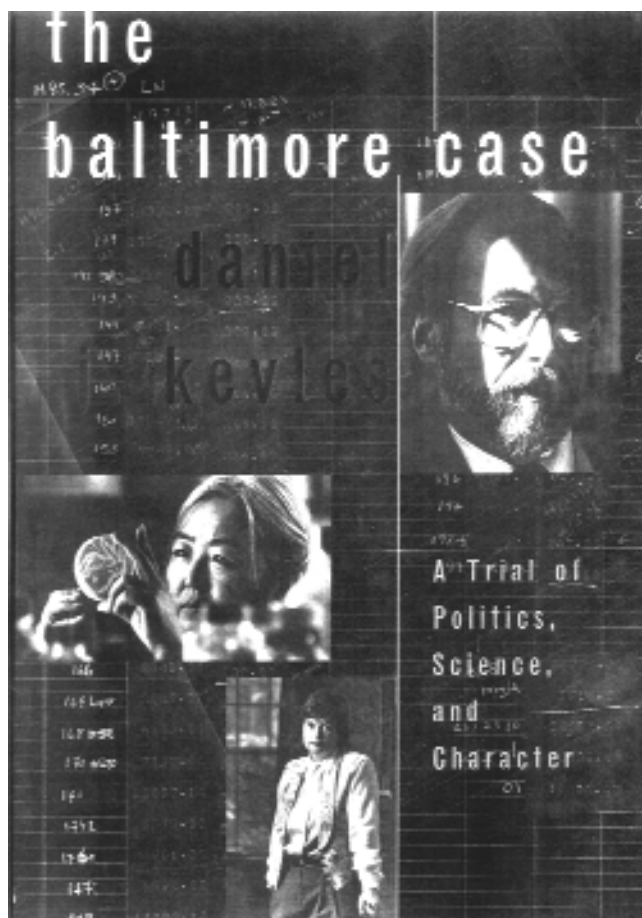


Fig. 1 Cover of the book *The Baltimore Case. A Trial of Politics, Science, and Character*. Daniel J. Kevles. New York: W.W. Norton, 1998. 509 pp. ISBN 0-393-04103-4

However, he also shows that the initial investigations carried out at the coauthors' and whistleblower's universities were marred by disinterest, lack of experience, and institutional rivalry between competing centers.

Kevles's book is filled with references that reflect his careful historiographic methods of investigation. However, many references are to his own notes on telephone conversations held with the persons involved in the case. This raises the possibility that some unconscious recall bias may have influenced both their account of past events and his reporting of these interviews. Moreover, as he admits in the Preface (p. 12), he "eventually became persuaded that Imanishi-Kari was innocent of the charges against her", his conviction being reinforced by her much-delayed official exculpation, and by Baltimore's re-entry into public life as President of Caltech—where the author has been a member of the faculty for more than thirty years. So it appears the author had a mission in writing this book. That mission was not to discover the truth about the original publication in *Cell* [12] that caused the controversy, but to rehabilitate Baltimore and present him as a martyr to politically-motivated, incompetent government meddling in the subtleties of scientific research. The writing is consistently slanted in favor of Baltimore, and against whistleblower O'Toole, congressman Dingell, unofficial fraudbusters Stewart and Feder, and indeed all other players perceived by the author to be determined to bring his hero down.

Kevles thus combines his skills as a historian with a well-stocked arsenal of rhetorical devices to lead the reader to the conclusion that Baltimore was treated unfairly by his professional adversaries, the lay press, and the US government. And he succeeded in convincing at least one book reviewer and journal editor that his view of the story is the only one worth believing [10]: Steele's book review in *Nature Medicine* reflects the triumphant tone with which the scientific community celebrated Imanishi-Kari's official exculpation as proof that the government has no business trying to regulate research.

Judy Sarasohn, a Washington journalist, states in the Acknowledgments (p. ix) of her book (Fig. 2) that she set out to write "a story of human frailties and strengths for a broad audience", rather than to try attempt to judge who was right and who was wrong. She explains that all scientists who were aware of the Baltimore case were polarized, and that she found it difficult to find a neutral immunologist willing to explain the scientific basis of the disputed *Cell* paper to her. As she notes, "Many scientists had not bothered to, or did not want to, look at the actual paper and allegations in dispute, and their feelings about the controversy were so raw that they did not believe other scientists could be objective." This is a key point: the controversy became so heated that legitimate questions about the data in the paper were almost forgotten in the battle to save Baltimore's reputation, and to keep the government out of the laboratories. But much of her information is also based on personal interviews with the players, so again, readers need to be cautious. However, both authors recount the same events as

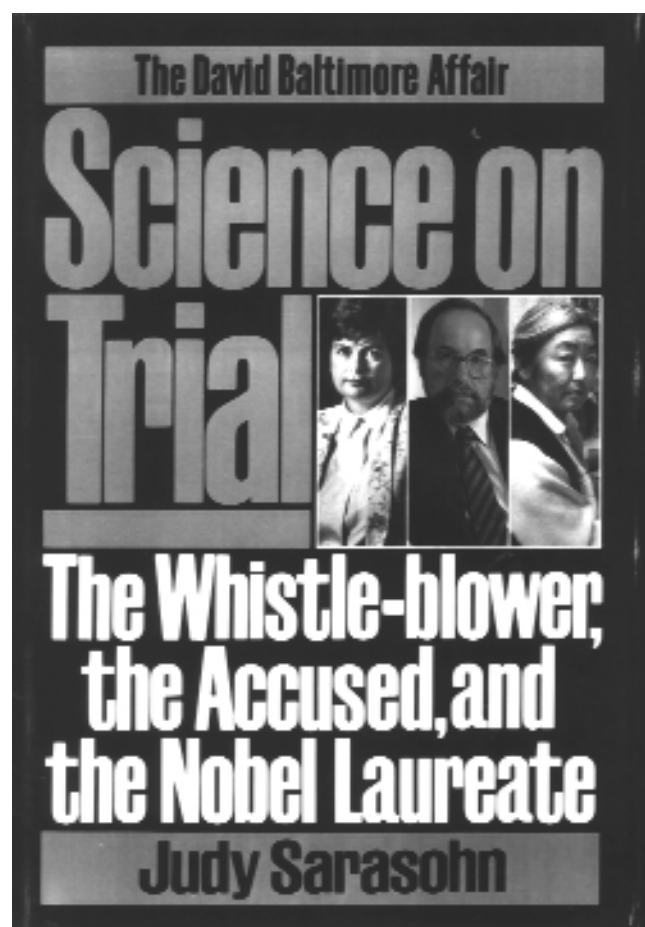


Fig. 2 Cover of the book *Science on Trial. The Whistle-blower, the Accused, and the Nobel Laureate*. Judy Sarasohn. New York: St. Martin's Press, 1993. 294 pp. ISBN 0-312-09247-4

reported by many of the same sources, and I found no discrepancies in their accounts of historical fact. Perhaps the main difference in comparison with Kevles is that Sarasohn points out how Baltimore's position regarding the flawed data in the paper, Imanishi-Kari's role in producing them, and O'Toole's motives in denouncing them, changed radically as the investigation proceeded. Sarasohn divides her analysis into nineteen chapters, followed by detailed notes on sources. Fortunately each book concludes with a well-constructed index that makes it easy to locate specific information.

Both books do an excellent job of presenting the immunological findings initially reported (then retracted, then unretracted) in the original *Cell* article, and of providing possible interpretations of their significance. Regardless of whether readers prefer the dense documentary tone used by Kevles or Sarasohn's true-to-life scientific drama style, all will appreciate both authors' careful dissection of the facts about the original data. Both books do a very good job of explaining the technical problems with the data in the original paper. Neither author

disguises the fact that the dispute between coauthors and the whistleblower went beyond mere differences in interpretation, and eventually turned on serious—and entirely justified—doubts as to the accuracy of some of the data. In fact, the appeals panel of the US Department of Health and Human Services (DHHS), in its final decision, noted that “The *Cell* paper as a whole is rife with errors of all sorts... [including] some which, despite all these years and layers of review, have never been previously pointed out or corrected” [5]. One can’t help but wonder why these errors were not detected by the journal’s peer reviewers.

Although both books recount the same events, Sarasohn’s brings readers closer to appreciating how scientists, administrators and policy makers became caught up in an unprecedentedly intense storm of controversy, and how their conflicting motivations allowed questions raised about the accuracy of findings reported in prestigious journal to escalate into a national crisis of trust in publicly-funded research. Sarasohn seeks to trace the emotions behind the players’ actions, whereas Kevles analyzes the case from a more political and legalistic viewpoint. (Photographs of Imanishi-Kari’s lawyers are among the many faces that illustrate his report.) In either case, the moral of this long and troublesome story for researchers is “Keep accurate lab notebooks, and discuss the data with all co-authors and colleagues whose unpublished observations are cited, before submitting the manuscript.” If Imanishi-Kari had followed the first piece of advice, and if Baltimore had been more conscientious in following the second, the whole affair would very probably not have happened.

The many errors made in the Baltimore case investigation, and the painfully long period (about ten years) that had to elapse before the final ruling was announced, were the result not so much of intentionally destructive government intrusion as of the lack of experience at all levels of administration and oversight in dealing with accusations of improper behavior. When O’Toole first expressed her misgivings about the data, neither universities nor the US government had at that time clear procedures in place to follow up on allegations of misconduct. In the end, the need to guarantee due process to the accused took precedence over the evidence—much of it eventually disallowed on technical or circumstantial grounds—that Imanishi-Kari and her co-authors published data that were of doubtful validity, initially refused to acknowledge that there were problems with the data, and intentionally provided inaccurate information to the OSI. (The actual course of events was much more complex, so don’t take my word for it—read the books yourself!) This outcome was entirely laudable and necessary in a system in which due process must take precedence over any other consideration, and Kevles reiterated this point in his response to Turney’s book review in *Science* [6]. It nonetheless failed to get to the heart of the problem that triggered the controversy in the first place: Should those data have been published? How did a paper with misinterpretation of the data, serious errors, and internal inconsistencies get into

print in a prestigious journal? An appeal to the DHHS eventually exculpated her of all counts of misconduct, but after reading the books, one wonders what the outcome would be now that more formal mechanisms exist to investigate allegations of misconduct and academic abuse of power. On the basis of the information Kevles and Sarasohn have given us, it is wrong to conclude that the allegations O’Toole brought against her superiors were “spurious,” as remarked in a Commentary published in July 1999 in *The Lancet* [3].

This and other highly publicized misconduct investigations had consequences for journal publication policies. Many journals implemented stricter criteria for authorship, and began to require that all co-authors and colleagues whose unpublished work is cited give assurance in writing that they have read the entire manuscript and agree to its submittal. In addition, journals began to require that the original data be kept on file to be made available to the editors or reviewers on request, if doubts should arise as to their authenticity. Another change in submission policy that seems to have stemmed from these cases is the requirement that authors give assurance that figures, especially those that have been subjected to photographic or digitalization processes, have not been manipulated to enhance or delete certain features.

However, despite these prophylactic measures aimed at preventing fraudulent information from entering the peer review system, journal editors have almost unanimously refused all responsibility for detecting or investigating suspected fraud. The argument is that submission of manuscripts and peer review are undertaken in good faith, on the assumption that the contents of every manuscript reflect honest findings of legitimate scientific inquiry. Therefore, if the research looks plausible, reviewers and editors have no motive to suspect misconduct, and are therefore not primed to detect it. Such a policy constitutes a tacit admission that journals are sometimes hoodwinked into publishing unreproducible observations. This, in fact, is apparently what happened with the *Cell* paper—inconsistencies and contradictions in the data went unnoticed (or were overlooked) because the overall picture of the findings and conclusions seemed reasonable.

Some editors will alert the author’s immediate institutional superior if questions are raised about the reported work in the course of peer review, and suggest that the matter be investigated through the appropriate channels at the authors’ home institution—often ignoring the fact that no such channels exist yet in most universities and research centers in countries where English is not the first language. (The only exceptions to date are four Scandinavian countries which now have institutional mechanisms in place to investigate allegations of misconduct [9].) To compensate for this hands-off attitude, many (but not all) editors and publishers now accept their responsibility to report findings of misconduct that affect articles that have already appeared in their pages, and publish retraction or correction notices if they are informed that the data have not withstood scrutiny [1, 2, 4]. Anti-misconduct measures

implemented by journals are thus sometimes effective in drawing the readers' attention to the fact that the horse has escaped, but may have little effect on ensuring that the barn door is locked in the first place.

As a result of the US government's investigations into notorious incidents of alleged scientific misconduct that came to official attention during the 1980s and early 1990s, institutions requesting NIH funds for research were obliged to develop formal ethical and professional guidelines specifying the levels of responsibility for investigating dubious research and publication practices, and abuse of power within the academic hierarchy. This has probably been the most important consequence of the scandal for publicly-funded science. Academic centers have been urged to commit themselves to developing their own rapid, fair, and transparent procedures for deciding whether abuse has taken place. In most cases, this change in policy has not occurred spontaneously, but has been a reaction to the perceived threat of government interference with the conduct of science. However, issues of authorship, legitimate scientific disagreement or error, good laboratory practices, and ethical publication practices continue to create problems. The economic and professional circumstances that lead researchers to resort to ethically questionable tactics in order to publish often and ahead of their competitors, and to receive continued funding for their work, have not changed. Although universities now appear more willing to punish delinquent staff who have been caught after the fact (often simply by terminating their research, while allowing them to keep their teaching post), evidence is still needed that institutional procedures for investigating their own employees are actually effective in preventing or changing unethical behavior before it occurs.

Countries and transnational organizations that often look to the US or UK governments for models on how to administer research funds, reward productive scientists and handle misconduct investigations should consider their own cultural and historical differences in research practices before they attempt to import solutions that have not yet been proved effective, without due regard for cultural and historical differences in research practices. It may be better in the long term to educate researchers about professional ethics [8], although it will always be necessary to have a well-designed system in place to define, detect and sanction abuses. Researchers, like other professional collectives, must now accept

that accountability needs to form a key element of their work ethic.

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Interesting websites

www.nyx.net/~wstewart Walter Stewart's site on research fraud and misconduct

<http://ori.dhhs.gov> Department of Health and Human Services site. Contains the text of the reformed procedures for investigating allegations of research misconduct

www.acponline.org/journals/resource/unifreq American College of Physicians site (publishers of the Uniform Requirements). Contains updates of the original document in the form of Additional Statements, which provide professional and ethical guidance on publication issues